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AUTHOR Engel, Walburga von Raffler
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ABSTRACT

This paper is essentially a critical survey of different theoretical approaches to child language in general and to child phonology in particular. The author states her own conviction that language acquisition is different from language retention, that language acquisition is a non-unique process consisting of a multiplicity of devices, that language is essentially habit behavior, that phonology represents a separate language level, and that hearing is not conditioned by articulation. In considering various other theories of the child's acquisition of phonology, the author questions the theoretical foundations of much psycholinguistic research. She is especially concerned that psychological facts and measurements be kept separate from linguistic ones and warns against a tendency which she finds among many researchers to identify method with subject: "The fact that a linguist is able to organize his linguistic data into a coherent system does not necessarily mean that an infant acquires, stores or recalls these data in the same manner as evolved by the linguist." She feels that a great deal more research must be done and that much more empirical evidence, especially in the way of quantitative information, is needed before valid theories can be formed. (FWB)

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Theoretical phonology and first-language acquisition: How scientific is psycholinguistics?

Walburga von Raffler Engel
Vanderbilt University

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Before talking specifically about the various theories of the child's acquisition of phonology, I wish to make some preliminary remarks on the concept of "scientific approach" to this, and for that matter, to all other problems in linguistic theory. The issue is quite basic. It is controversial, but, more than that, there is a great need for clarification.

Beyond a general acceptance of MacPherson's definition of the scientist as one who "tends to believe what he has seen or what other trained observers have seen, provided that all such events fit together in a natural way and can be correlated by general rules,"[1] there exists a considerable amount of confusion about what constitutes the proper object and what methodology is to be followed in a scientific inquiry. The general state of confusion may explain how at the same time Theo Vennemann can claim that Chomsky's "conception raises linguistic theory from the level of an intellectual game to that of a scientific discipline,"[2] and Robert Hall can criticise "the anti-scientific character of Chomsky's basic tenets" and state that this "aprioristic rationalism and all its consequences . . . must be returned as quickly as possible to the limbo of outworn dogmas, and linguistics must return to its basic observation of human activities in relation to their culture, if it is to continue developing as a science."[3]

Everybody agrees upon - and, as far as I can tell, honestly

attempts to follow - a rigorous and logical methodology, i.e.one by which each subsequent step of operations is based on the preceding one in an orderly sequence of cause and effect. Some linguists, however, have overexpanded the concept of logic beyond a legitimate means for the analysis of language to imply that language qua object of inquiry ought to be logical by sheet definition. This has led to logical - but false - deductions. The analyst must insist on the separateness of linguistics and logic. "To the argument that the double negative, such as 'I don't see nobody,' cannot be used together to furnish a negative meaning, because two negatives make an affirmative," Joseph Greenburg "will point out that ancient Greek does just that."[4])

The greatest danger of identifying one's method with one's subject lies in the emergence of tautologies, such as the following from The Sound Pattern of English:"The principle of the transformational cycle being well beyond the bounds of any conceivable method of 'learning' is one of the conditions, intrinsic to the language acquisition system. . . . Notice, however, that the transformational cycle might apply vacuously in a certain language, in particular if the language has a very shallow surface structure."[5])

But even within the realm of methodology, there is a tendency to equate what properly pertains to the analysis of the data with the heuristics of the gathering of these very same data. Mathematical - i.e.algebraic - models are fine for analysing one's findings, but certain primary facts can be computed by statistical methods only. To discover if there is any consistency and if so, which phonemes are first produced by all children and/or are universally present in all

languages, quantitative - i.e.arithmetical - methods are needed.

Mathematical models are excellent because they show things with the utmost clearness. But such models should remain tentative until the secondary data - i.e. those collected to crosscheck the validity of the model - prove its validity. By validity, I mean the absolute validity which does not allow for exception. If we grant that "exceptions" are part of all that is human, we may make a perfectly legitimate statement, but not a mathematical one.

A theory of naturalness which posits the appearance of the pertinent stop before that of an affricate in child phonology is sufficiently contradicted by the few cases to the contrary that come readily to mind. My own son's first word was ciao [6] By definition, general theories are exceptionless. "All men are mortal" states just exactly that.

According to universal conventions, š is more marked than s.[7] Would this imply that children acquire the dental fricative before the palatal? One example to the contrary is Martinet's daughter, whose first word was cochon.[8]

The current idolatry of neat designs, confusing the experimental method with the scope of the experiment, has led some researchers to identify an artificially created laboratory situation with the live facts of natural language. In this fashion, drawing conclusions from the arbitrary isolation of a sentence to our normal context-bound speech, it has been asserted that a sentence like the boy was struck by the bridge is "disambiguated" by a process of gradual steps of back-derivations. All of these transformations are supposedly identical for the speakers of the same language. In natural language strike is one of many multiple-meaning verbs. The above sentence is hardly ever ambiguous. It is clear

from the extra-linguistic situation and/or from the preceding language context: John is a great admirer of architecture and when he saw the Golden Gate,...; or Jim has always been accident prone and when he drove under ... There is no cogent reason for ruling out sequential understanding. In one context strike is interpreted as equivalent to wounded while in the other context it can be substituted by awed. If the context is not immediately clear enough we may have to go back to it after listening to what follows, like and is still raving about its beauty or and is still at the hospital. Suprasentential substitution is practiced also by children. An example from the active language of a 27 months old child is the one single word covering two lexically almost synonymous, but syntactically very distint items, pray-be for both probably and maybe.

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Mathematical models must not only be validated after they are drawn up. They ought, to begin with, be applied only to the specific field of inquiry they are to cover. The purpose of algebraic formulations being one of precision, it is improper to create a model for a broad area beyond functioning like a chart in order to show the organization of various sub-models as they relate to each other. If all the data, instead of being filtered through narrow models for each specific type of groups of facts, are randomly put into a broad model, the outcome will probably be incorrect, or at best, trivially vague. On the analogy of Sigmund Koch's

"Psychological Studies"[9] I would like to speak of "linguistic studies," not as our ultimate goal, but at least for the time being. If, out of necessity, we have to be vague, let's be frank about it and not immodestly build a pseudo-science. We just do not have the complete list of facts for a formal system of linguistics. In the area of the infant's understanding of language we even lack the proper tools for our research.

In much of pedolinguistics we have just started refining the formulation of the pertinent questions, and new problems to be investigated are showing up all the time. Jensen's concept of the triple interaction among the variables intelligence, associative learning ability, and socio-economic status[10] may eventually reveal itself useful to the study of language acquisition. Could hereditary features play any part in the rapid acquisition of the phonology of the language and/or in the child's power to pronounce more clearly at an earlier age than most of his peers? Tentatively, from our present scanty state of knowledge, most linguists would answer in the negative. But we do not really have well organized statistical evidence, particularly on the latter issue.

Quantitative information is sorely needed in linguistics. When speaking of the adult grammar to which the child is exposed, Chomsky and Halle state that the "primary linguistic data are, in large measure, ill-formed, inappropriate, and contrary to linguistic rule."[11] The assertion is inaccurate on statistical grounds. It is conceivable - but not certain - that on an average day we are likely to utter more ungrammatical sentences than fully well-formed ones, but our half-finished sentences and our anacoluthons do not form a

consistent pattern. (If they do, by the way, and ungrammaticality is congruent, it eventually becomes part of the regular system.) The child is exposed to random inappropriatenesses and to consistent well-formedness. The cumulative quantity of the latter, taken as a model for analogical formations and/or rule deductions, by far outweighs the absolute majority of the former where each instance is a nonce. In phonology, it is well documented that children imitate a parent's consistent speech defect. Some children had to undergo speech therapy to lose such an acquired habit.

Most of all, no problem in pedolinguistics can be adequately approached as long as there is no clear distinction between psychology and linguistics. It may very well be possible to chart phonological data on a plus-and-minus basis, but it is certain that the human brain does not operate with binary choices.

Much of recent work in child language is viciated by the assertion that the only valid grammar of a language is the one which reflects both first-language acquisition and the native speaker's competence. That the last factors are to be identified is assumed but never clearly established. On the contrary, according to transformational theory, new linguistic information is supposed to be incorporated in a different manner by children and adults. The former are said to modify their grammar by alteration of rules while the latter increase their grammar by rule addition. The contradiction is explicit.

Without going into the details - most of which are unknown, anyway - of mental operations, it appears that the acquisition of language is a process and the retention is more similar to a state. Any identification of the two is inaccurate. Furthermore, neuro-

pharmaceutical research has demonstrated that learning and memory are not connected with the same bio-chemical reaction. To combine even one of these diverse operations with the correct linguistic description of a language is absurd. That diachronic and synchronic analyses cannot be described in an identical fashion was clearly shown by Joseph Malone.[12)]

The same author, in a subsequent article, has also adduced convincing evidence in favor of the non-uniqueness of linguistic solutions[13)] against the opposite view represented by Sanford Schane. Schane proposes that "non-uniqueness be expunged from linguistic description as all such issues can be solved in a unique manner by the application of the markedness principle."[14)] The entire problem of non-uniqueness, to my mind, is even more complex than the aspects dealt with in the above controversy.

Logic describes a manner, psychology a behavior, and linguistics deals with overt data and their systematic organization. Psycholinguistics is to discover the behavior - conscious and unconscious - underlying the linguistic facts and systems.

As Malone has documented the non-uniqueness of interpretation in linguistics, so Alexander Hull, in a psychological interpretation of some linguistic data, has proposed that "language may not possess a single grammar, but rather a number of competing potential structures, no one of which in pure form can be posited as the competence of any speaker. The language behavior of a given person will represent a compromise among these, varying in accordance with the educational level. . . ."[15)] Hull's paper proves to me that speech programming is non-unique, and, to a certain extent, idiolectic. The extraordinary

complexity of diasystems is felt most sharply by the compilers of linguistic atlases.[16] What I am planning to do is to show that language acquisition too is non-unique. It is my hypothesis that in the process of language learning there exists not a unique type of memory coding, but a multiplicity of devices. The consequences of differences in the learning situation are far more lasting than is commonly believed. The color spectrum is divisible in a variety of ways, all equally scientific. In physics we use light waves. In Hanunóo, colors are coded depending on "certain correlates beyond what is usually considered the range of chromatic differentiation, and which are associated with nonlinguistic phenomena in the external environment."[17] There is, among others, "an opposition between dryness or dessication and wetness or freshness (succulence)."[18] If a native speaker of Hanunóo happens to be a trained physicist, he can certainly describe the colors in terms of light waves while at the same time perceiving them according to the color system of his culture. Anthropologists generally do not identify perception and analysis. Why should linguists?

Theories have been built upon contrasting motor sounds, imitative babbling, and selective creativeness. None of these theories explains the messy real life situation. Such a complex instance occurred in the speech development of the son of a student of mine, one of whose babbling sounds was gigl. The baby's father "took a liking to that sound and, when he heard it, would go and play with the infant, all the while repeating David's babbling. The verbal aspect of the communicative relationship established was restricted to gigl. Eventually, David came to associate gigl with his father, and whenever

he espied his father, he would revert to the signal gigl."[19] What this amounts to is non-imitative holophrasis.

Concerning the classical dichotomy according to which "One might look at single morpheme utterances from two widely different point of views: they are stored in memory as a sequence of sounds which are inseparable and these sequences are verbal symbols of auditory, visual, or tactile images; or they are stored in memory as the syntactic structure sentence with semantic properties and phonological features to which intonational markers, also stored in memory, are applied as these sequences are generated,"[20] it is assumed that even though the two points of view are in opposition within linguistic theory, in the practice of language acquisition, they are co-existent.

Whatever the merits of the diverse linguistic theories, it is not possible to equate any one of them, in its entirety, with the working of the human brain.[21] The fact that a linguist is able to organize his linguistic data into a coherent system does not necessarily mean that an infant acquires, stores, or recalls these data in the same manner evolved by the linguistic theorist.

I am convinced that the only avenue to an understanding of the relationship of cognitive development and linguistic structure lies in a series of narrow quantifications. From these discrete entities we should then try to put the whole together. In this manner it is more likely that some of the variables can be detected. It will be a painstakingly slow and thorough work; and hopefully will uncover a web of underlying subsystems as well as their connecting lines - direct and/or indirect - to the overall system.

What I have said so far is based essentially on two notions. One is the distinctiveness of psychological and linguistic facts and measurements. The innateness theory explains away this separateness to some degree, but so far the latter is only a convenient working hypothesis and as of yet has neither been properly researched nor have its proponents succeeded in refuting the basic objections moved against it.

The other notion primarily separates the description from the described. Quantification carries us into the philosophical debate on artificial intelligence that is currently going on between Harvard and M.I.T. Underlying the M.I.T. school is a "conception of man essentially rational, and rationality as essentially calculation."[22] The digital approach to psycholinguistics is "necessarily committed to . . . the ontological assumption . . .that everything essential to intelligent behavior can in principle be understood in terms of a determinate set of independent elements."[23] Thinking is conceived as "data processing - a third person process in which the involvement of the processor plays no essential part."[24]

On the other shore, there is Anthony G. Oettinger, concluding that "our discrete enumerative approach is doomed."[25] Oettinger was led to this pessimistic conclusion when he discovered that the understanding of a sentence proceeds from meaning to structure.[26] The idea is not new and, as early as 1907, the Sterns observed that in child language the whole (the sentence) precedes the part (the word).[27]

My own research has led me even further in that same direction, concluding that the child proceeds from the entire discourse.[28] But I do not feel that evidence of gestaltist perception conflicts with

the possibility of quantification. I am in general a follower of Martinet's functionalism which, like all of structuralist linguistics, separates accidence from essence. How this capacity is acquired by the child, we do not know and, so far, we have not been able to simulate it on machines. Dreyfus may be right that "The ability to distinguish the essential from the inessential seems to be a uniquely human form of information processing."[29] I would go even further in saying that it is this ability which is essential to life and is present in men and animal and probably an inborn vis vitae. In any case, our ignorance of the mental process of the perception and understanding of language has not prevented us from separating -etic and -emic elements in linguistics.

Clinicians working with schizophrenics are generally aware of semantic functionalism as a requisite for normalcy. Schizophrenic patients are "not able to learn to screen out the specific irrelevant stimuli which are occasionally associated with the common elements which define the concept."[30] Cromwell and Dokecki call this the "disattention deficit."[31] Their documentation is rigorous, but not mathematical. They come to say that their findings "would not represent acceptable scientific propositions. However, like any subjective or phenomenological proposition their value lies only in the testable predictions they mediate."[32] Is their research unscientific?

There is no question in my mind that the child's language - both active and passive - originates to fulfill a need.[33] This seems to be sufficiently documented from the baby's cry[34] to the appearance of Wunschwoerter.[35] How far we can go in mechanical simulation, I do not know. A machine, probably, "like a disinterested

observer, has at best, specific targets rather than needs."[36]

In this connection one is reminded again of Koch's "Psychological Studies,"[37] In the realm of the child psycholinguistic behavior, for example, while there is continuity in the infant's need and in his attempts to verbal satisfaction, in his understanding there is a break from intonational dominance to articulatory dominance, with subsequent addition of syntactic clues.[38] Phonetically, babbling leads to imitative prattling, but viewed from the standpoint of expression, the same data are to be considered as divided by a sharp break, overlappings not withstanding.[39]

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The two basic points made above, of the primacy of discours and of the developmental discontinuity of infant communication, in themselves do not automatically contradict the evaluation principle.[40] Such an evaluation procedure, however, ought not to be digital. If this is conceivable - in practice as well as in principle - I do not know.

Strictly binary choice making does not seem to characterize the natural processing of human language. That antonyms are never direct opposites is well known. But not even phonological oppositions are that simple. The difference between English p and b is not just one of voicing but also one of lack of aspiration when in initial position.

Children do not acquire language algorithmically, and memory storage appears more like a map. Learning is an experience and new facts are acquired not only for what they are but also for what they represent in relation to one's previous knowledge and, as any translater can tell, the learning process itself constitutes additional

knowledge. What is acquired ultimately is at least threefold. Essentially, language is habit behavior.

A three-year old child can acquire a foreign language faster and better than an adult by simply being exposed to it. But it is impossible to teach grammar rules. He has neither the power to comprehend them nor the patience to listen to them, to begin with. Children acquire their first language and, whenever the case, their second and third languages, by "resonance." The Cazden report[41] has clearly supported my findings of nine years ago.[42] Keeping in mind the distinction made earlier in this paper between the description and the described, we are free to assert that if language is not acquired completely by rules, this does not prevent a description which organizes the material according to rules. The issue, however, is considerably complicated by the evidence from the Berko Morphology Test.[43] It is entirely possible that cumulative analogizing may eventually lead to rule governed coding.

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Leaving the problem of rule formation unsolved, I would now like to discuss the simplicity principle.[44] The linguist can present a most concise analysis and, given the non-uniqueness principle, there are more than one possible such sketches. He can develop general overall rules for the whole language or for just one level of analysis.[45] But to posit a simplicity matrix as part of the language acquisition device amounts to pure fiction. Such a conception does not distinguish between logic (and mathematics), linguistics, and psychology. It identifies the learning, storing and retrieving of information with the information itself, the description with the

described. And the child with the linguist.

Each speech sound can be acoustically described in terms of the frequency and amplitude of its formants. The sound is produced by a combination of articulatory features. The neural motor impulses for each of these features continue until no longer required in the speech chain. In the word bookshelf voicing goes on until we reach the k and is not continuously present from k to sh. The airstream comes out uninterruptedly throughout boo and again from sh all the way to f. The tongue stays in the same fronted position for sh and e; etc., etc. This distinctive feature continuity would go on if instead of bookshelf we said bookshelf stacks or bookshelf stacking order in one breath group. It is the speech continuum quality of their distinctive features which marks these sounds as true phonemes of English. The articulatory components of marginal phones, such as in English the dental click or the implosive of astonishment, cannot be continued. The distinctive features of the South African clicks, which are phonemes, on the contrary, function as phonetic long components. The treatment of phonemes and marginal phones is so totally different that they cannot possibly be charted together in a phonological analysis. Neither can any markedness principle be applied in this situation. Universal markedness is still an open question anyway;[46] and language-specific neutralization is not to the point.

What this phoneme/marginal phone contrast may signify in the controversy over the traditional phoneme versus the underlying lexical representation, I am not clear yet. What is apparent, so far, is that an almost identical sound, in one language has "open"

distinctive articulatory features, whereas in another language the combination of its distinctive features form a closed unit.

To conclude, there seem to be phonemes which are coded by their distinctive features[47] and phonemes which are psychologically indivisible. Further research is likely to reveal that there are syllables which psychologically cannot be broken into their constituent sounds. Phonemic analysis, particularly of the contrastive type,[48] has proved of considerable usefulness in child language. I cannot, however, assert conclusively the validity of phonemic theory in child phonology. But that orthography is easier for children to read than a phonemic transcription[49] I have good reason to doubt.

My son was raised bilingual, Italian and English. All of his formal schooling, however, was in the United States. Although he could speak Italian, he had never attempted to read it until, at eight years of age, during an extended summer in Italy, he became curious about the names of the bathing establishments along the beach of the resort place we were staying. His name is Robert, and one day he suddenly noticed "Roberto" written over the entrance of one such establishment. From then on he proceeded to read all the names of each one of a row of bathing establishments. Subsequently there came street signs, and eventually books. The child did this first according to English spelling rules. And when this made no sense to him, he figured out how he had to read Italian, which has an almost phonemic spelling system. In the beginning, as was to be expected, he encountered certain difficulties. Interestingly, whereas he learned very quickly the value of gn, he had much trouble

with ch even after he no longer associated it with the English ch of church. What worried him was the fact that two graphemes ch represented one simple sound k. Of this he spoke to me at length. When I reminded him that English did the same thing in the spelling of sh, ne considered all of it quite "crazy." He did not seem to have a lexical representation that would associate Italian c and ch, alternating in such common words as amici/amiche.

The study of child language has confirmed my belief in the phonological level as separate and not only separable.[50] Natural language is a semiotic system which is complex, ordered, and doubly articulated (into phonological elements and signs).[51]

Acoustic cues are perceived by the hearer depending on his semantic expectancies. Underlying lexical representations fail to explain folk etymologies and slips of the tongue. Children's experience being more limited than adults', they are more prone to misinterpretations. In virtually each family there are fond memories of their children's imaginative mishearings, some of them very funny. When my little boy was seven years old, he seemingly did not know the word only. And only child was rendered as a lonely child. At 27 months of age, the daughter of my typist says overals for overalls, probably on the analogy of manuals and the like.

Concerning the motor theory of speech perception, in my opinion, hearing is not conditioned by articulation.[52] In regard to the Jakobson-Lebrun controversy, I believe that a child's phonological substitution is due to his inability to perceive phonemic contrast. A student of mine has researched the issue and concludes, with Lebrun, that "The child perceives the phonemic contrast but cannot perform the delicate and specific movements needed to articulate a certain phoneme in context. The child, who apparently is

not aware of his lack of articulatory skills, produces what he thinks is that phoneme."[53] At age 3½ my son was actually aware of his articulatory imperfections and tried to avoid words containing difficult phonemes.[54]

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The more one delves into the acquisition of phonology by the child, the more one discovers of controversies and unsolved problems. In addition, new problems are constantly coming up and have to be posited. As one starts investigating, many problems seem to get more remote from any possible solution, increasing only in complexity. Could this be due to lack of clarity in our basic formulation?

There are at least four different - and equally legitimate - approaches to child phonology. The sounds are the same, but the ways they offer themselves to analysis are of different kinds. And this does not only imply the analysis of the sounds as such as compared to their relationship within a system. That phonetic and phonemic relationships are different has been established long ago and, although rephrased in other terms, is recognized within generative phonology. I venture to conjecture that the depressing state of research in child phonology may be due to a general lack of distinction of the methods and purposes of the various types of approaches. Psycholinguistics sometimes seems to look for a mixed solution which inevitably becomes a mixed-up solution. The need for clear-cut research proposals is the point of what I said before. Only after we establish these can we proceed to work profitably.

From the acoustic standpoint sound analysis is clear and amenable only to the improvement of mechanical instruments. The

articulation can be described quite objectively. The Haskins research on pre- and post-voicing and other break-downs in the hierarchy of articulatory distinctive features will eventually allow for a full physiological analysis. Linguistically, the material can be represented under the tenets of one theory or another with equally good reason. A psychologically valid description of the process - underlying and overt - is different from the other three approaches and should be neither confused nor subordinated to them. A tentative project for research in this area will be the subject of my next paper.

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